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MAN AND HIS NERVOUS SYSTEM IN THE WAR

BEING SOME REFLECTIONS UPON THE RELATION OF AN ORGANISM TO ITS ENVIRONMENT

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ADMIRAL SIR JOHN JELLICOE, in the dark months of the past year, advised his countrymen to look at a map, and, furthermore, to look at a large map if they wished to get a true perspective of the war. I would like to present a picture for inspection, and, furthermore, I would like to present a large picture, as I believe that in a large picture we may find comfort. In a large picture, some of the lines may be blurred and indistinct when examined microscopically, but the perspective is better. And when once we get a perspective, we may elaborate detailed portions of the picture to the limit of microscopic vision. The picture, to be sure, is, in part, drawn after the events have occurred, and to this extent now represents afterthought rather than forethought. But if I mistake not, some of the conditions which have given rise to trouble in the past will greatly outlast the war, and a recognition of the sources of evil in the past may help us in avoiding, or in coping with, the sources of evil in the future. Assuredly, man's mind is in need of comfort, and while one does not ordinarily look to the biologist for comfort in times of adversity or trial, I hope to be more successful than the officious gentlemen who called on Job.

Some, like Mr. Britling's son Hugh, have tired of the war at times and wished for stories of gods and heroes so that they might forget for the moment the horrors of the conflict. I have at times been one of them. But not until the news of the signing of the armistice arrived was I able to sit down and read with untroubled mind anything that did not have some more or less close connection with the war and the part which I was trying to fulfil in it. For those who may have felt as I did at times, but who still look forward with some anxiety to the formulation of the peace terms and the statement of the means adopted for curbing some of the vanities of human ambition for the future, I wish to show that the study of the effect of war and war conditions upon man is a biological study of the effects of the environment upon organisms and that if the essential relations of biology to the life of man had been fully recognized this war might not have come. I wish to present a statement that I have worked out in my own mind in the terms of my chosen science, physiology. To be sure we are dealing with one species of organisms and not with organisms in general; and the environment bears the trade mark, "Made in Germany." But biology is essentially an inductive science, or group of closely related sciences, rather than a deductive science, and inferences drawn from a study of one group of organisms will fit somewhere in the general scheme of inductive thought when it is finally worked out. I will first map out the general field of physiology as it has been developed up to the present, and then show how the information now available may be applied to some of the problems of the war.

THE PROGRESS IN PHYSIOLOGY IN THE PAST CENTURY

The Place of Physiology in Scientific Thought.—For the benefit of those who may vaguely wonder, as some of my friends have wondered at times, what physiology has to offer to general biological thought or to scientific thought as a whole, a brief statement of the progress of physiology during the past century may not be out of place. Merz[1] speaks of the conflict early in the past century between vitalism and the mechanistic conception of life processes as one of the fundamental phases in the development of physiology, and attributes a large share in the founding of physiology on the mechanistic basis to Johannes Müller. Merz quotes Emil Du Bois Reymond[2] as follows:

> The modern physiological school with Schwann at its head, has drawn the conclusions for which Müller had furnished the premises. It has herein been essentially aided by three achievements which Müller witnessed at an age when deeply-seated convictions are not easily abandoned. I mean first of all, Schleiden and Schwann's discovery, that bodies of both animals and plants are composed of structures which develop independently, though according to a common principle. This conception dispelled from the region of plant-life the idea of a governing entelechy, as Müller conceived it, and pointed from afar to the possibility of an explanation of these processes by means of the general properties of matter. I refer, secondly, to the more intimate knowledge of the action of nerves and muscles which began with Schwann's researches, in which he showed how the force of the muscle changes with its contraction. Investigations which were carried on with all the resources of modern physics regarding the phenomena of animal movements, gradually substituted for the mir-

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[1] "A History of Thought in Europe during the Nineteenth Century," I., p. 217, Edinburgh and London, 1903.

[2] Reden, Vol. II., p. 219.

> acles of the "vital forces" a molecular mechanism complicated, indeed, and likely to baffle our efforts for a long time to come, but intelligible, nevertheless as a mechanism. The third achievement to which I refer is the revival among us by Helmholtz and Mayer of the doctrine of the conservation of force. This cleared up the conception of force in general, and in particular supplied the key to a knowledge of the change of matter in plants and animals. By this an insight was gained into the truth that the power with which we move our limbs (as George Stephenson did those of his locomotive) is nothing more than sunlight transformed in the organism of the plant: that the highly oxygenated excrements of the animal organism produce this force during their combustion, and along with it the animal warmth, the "pneuma" of the ancients. In the daylight which through such knowledge penetrated into the chemical mechanisms of plants and animals, the pale spectre of a vital force could no more be seen. Liebig, indeed, who himself stood up so firmly for the chemical origin of animal heat and motive power, still retains an accompanying vital force. But this contradiction is probably to be traced to the circumstance that the celebrated chemist came late, and as it were from the outside, to the study of the phenomena of life. And even Wöhler still believes in a vital force, he who in his time did more than any one to disturb the vitalistic hypothesis through his artificial production of urea.

Merz later admits that the French physiologist Vicq-d'Azyr, who, by the way, was a professor in the Ecole veterinaire d'Alfort and a neuro-anatomist of considerable ability, had, earlier than Johannes Müller, clearly stated the mechanistic conception of life processes. Merz[3] quotes from Du Bois Reymond[4] this extract from Vicq-d'Azyr:

> Quelqu' étonnantes qu'elles nous paraissent, ces fonctions (viz., dans les corps organisés) ne sont-elles pas des effets physiques plus ou moins composés dont nous devons examiner la nature par tous les moyens que nous fournissent l'observation et l'expérience, et non leur supposer des principes sur lesquels l'esprit se repose, et croit avoir tout fait lorsqu'il lui reste tout à faire.

One may remark in passing that, despite his recognition of the relation of the cell theory to the Aristotelio-Drieschian conception of an entelechy, Johannes Müller[5] still remained much of a vitalist. So deeply was the vitalistc theory ingrained in the minds of physiologists and others that Du Bois Reymond years afterward remarked that behind such terms as cell autonomy there lay concealed the thinly veiled specter of vitalism.

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[3] *Ibid.*, p. 219.

[4] Reden, Vol. II., p. 27.

[5] Although he himself (Müller) is truly regarded as the last of the vitalists—for he was a vitalist to the last—his successors were adherents of what has been very inadequately designated the mechanistic view of the phenomena of life. Burdon-Sanderson, J. S., *Nature*, 1893, XLVIII, p. 466.

Even after the discovery by Wöhler in 1828 of the possibility of producing synthetically such an organic substance as urea, such a universal mind as that of Johannes Müller was still clinging to the belief in the all-powerful force as the creator and harmonizer of the various mechanisms of the living body." Meltzer, S. J., *Science*, 1904, N. S., XIX., p. 18.

Merz's statement may appear insufficient to the physiologist, but I am merely quoting it to show just what impression physiology as a whole has made on the mind of a keen and diligent student of scientific thought in the nineteenth century. Compared with his statement on morphology, it is meager enough. One unfamiliar with the great names in physiology, and who did not look through the index for them would truly get an inadequate idea of the influence of physiology upon thought in the past century. Nor do the references to the physiological units of Herbert Spencer or to "physiological division of labor" really carry us much further into the place of physiology in scientific thought. For Milne-Edwards was an anatomist and comparative physiologist, and Herbert Spencer a philosopher. Their conceptions of processes or properties, however valuable they may have become in biological thought, can scarcely be claimed as the property of the workers in the technical laboratories of physiology. Howell[6] epitomized the situation by saying: "We must perhaps admit that the philosophical basis of physiology, its general principles and quantitative laws, have been borrowed in large part from other departments, and that the subject has not as yet fully repaid this indebtedness by contributions derived solely from its own resources." Nor does one find much mention of the relation of physiology to the other biological sciences in Verworn's article "Physiology" in the Encyclopædia Britannica. But if, as is generally alleged, physiology is one of the biological sciences, it must have some elements in common with them and hence some relation to the great questions of biology. It would seem permissible, therefore, to add something more on the place of physiology in scientific thought. We may first review briefly the progress of physiology, in order to get a background of fact upon which to base our conclusions, and then search particularly for those phases of the work which may serve to connect physiology with the great problems of biology in general. Those who are especially interested in the development of physiology should read also Professor Howell's address and those by Professor Burdon-Sanderson and Dr. Meltzer to which I have just referred. As will become apparent from what follows, the particular field of physiology has been the study of the individual and its internal conditions.

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[6] "Problems of Physiology of the Present Time." Congress of Arts and Science, Universal Exposition, St. Louis, 1904, Vol. V., p. 1.

THE STUDY OF THE INTERNAL ORGANIZATION OF THE LIVING ORGANISM

The work in physiology during the past century, and one should add twenty-five years to cover the beginnings in the eighteenth century and the continuation in the twentieth, has been along the lines of the chemical organization and a nervous organization. I will not enter here into a discussion of the moot question whether the nervous mechanism may not be at bottom a chemical mechanism. Assuredly, the nervous mechanism has some chemical properties, but the methods of investigation of the two systems are at present sufficiently different in character to warrant us in retaining the term nervous organization for a time at least. While investigation of the two systems of organization has proceeded more or less together, the development of the knowledge of each may be considered separately. Other writers might not choose the same facts or the same names that I have chosen, but I have taken those things which seem adapted for bringing out the particular points I wish to emphasize.

THE CHEMICAL ORGANIZATION OF THE BODY

Lavoisier, toward the close of the eighteenth century, showed that the heat production by an animal in a given time was directly proportional to the amount of carbon dioxide produced in the same time. This result led to the discussion of the nature of the chemical processes in living matter. It was generally admitted that the production of heat in animals was the result of a process of slow combustion, and the idea of a catalytic agent and of catalytic reactions soon followed. We have come to recognize more and more clearly that many of the chemical reactions occurring in living matter are of the same general nature as the "slow" reactions of the laboratory of physical chemistry.[7]

The line of investigation started by Lavoisier had other results. His original experiment in animal calorimetry was repeated with greatly improved methods by Regnault and Rieset, Pettenkoffer and Voigt, Atwater and Benedict and others, until the original discrepancy of a few per cent. between Lavoisier's observed results and the calculated results has been reduced to a few tenths of one per cent. From these experiments, and van Helmont's earlier experiment[8] on the increase in weight of a tree, the modern study of metabolism has arisen. Biologists generally regard metabolism as one of the fundamental properties of living matter. Goodrich's statement[9] in 1912:

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[7] Blackman, F. F., *Nature,* 1908, LXXVIII., p. 556.

[8] Foster, "Lectures on the History of Physiology," 1901, p. 133.

> The metabolic process in living matter draws in inorganic substance and force at one end, and parts with it at the other; it is inconceivable that these should, as it were, pass outside the boundaries of the physico-chemical world, out of range of the so-called physico-chemical laws, at one point to reenter them at another,

is a sufficiently close restatement of Vicq-d'Azyr's conclusion of more than a century before to justify the correctness of his physiological vision. That we do not yet know all the transformations of inorganic substance and force in the metabolic process is true. But to say that such transformations are not physico-chemical processes, as I have heard it said at various times, involves a peculiar mental operation which I am at a loss to understand. One might as well say that, because we can not explain the transmission of electricity along a wire—and I am not aware of any such explanation at present which is complete and wholly free from objection—such transmission is not a physico-chemical process. If vitalistic properties enter into metabolism at all, they can not consume more than a few tenths of one per cent. of the total energy involved in the process. To the other virtues of vitalism which Du Bois Reymond mentioned in his own humorous way, we must, therefore, add extreme economy of operation.

The artificial synthesis of many of the products at one time supposed to occur in living matter only, beginning with Wöhler's synthesis of urea in 1828, and extending through a long series of carbohydrates and even of polypeptids, is another series of achievements along the line of the chemical organization of the organism. Through the chemical study of the nature of the various compounds occurring in living matter, we have been able to extend our knowledge of the processes of metabolism in general, and of nutrition in particular.

The doctrine of the conservation of energy led to a new discussion of the position of life. Balfour Stewart[10] compares living matter to the class of machines whose distinguishing characteristic is their incalculability. He mentions also that "Joule, Carpenter and Mayer were at an early period aware of the restrictions under which animals were placed by the laws of energy, and in virtue of which the power of an animal, as far as energy is concerned, is not creative but directive."

Physiologists generally have considered this characteristic of incalculability in living matter under the head of irritability, and it has been recognized by biologists generally as one of the essential characteristics of living matter. It must be considered in any discussion of the internal organization of living organisms. Much misty terminology and vagueness of expression has clung about it, and the conception of irritability as Pfeffer formulated it has much of the element of vitalism in it. Blackman has expressed the hope that some of the implications of irritability will disappear from biology, and be superseded by a more modern statement in terms of chemical mechanics. As attention has been more and more focused upon it, the incalculability of deportment of living matter has been found to be little or no greater than that of high explosives in government storehouses, and one would have some hesitancy in attributing vital characteristics to explosives under recent diplomatic conditions. The term stimulus so often used in connection with irritability is another instance of a word which has no very definite meaning except a rather arbitrary one. It is my belief that the laws of chemical equilibrium are applicable, and will do much to clear up our idea on the subject. If, as there is now every reason to suppose is the case, the reactions in living matter are like other physico-chemical reactions, stimuli, which influence the reactions in living matter, are comparable to the conditions which influence ordinary physico-chemical reactions.

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[9] "The Evolution of Living Organisms," London and New York, p. 15.

[10] "The Conservation of Energy," New York, 1874, Chapter VI.

A discovery of considerable importance, as illustrating the action of a number of chemical substances in the body, was made by Bayliss and Starling in connection with the mechanism of eliciting secretion of the pancreas. These investigators found that when the acid contents of the stomach come into contact with the mucous membrane of the duodenum, a substance called secretin is formed in the mucous membrane. This substance secretin is absorbed by the blood and carried through the circulatory system to the pancreas where it excites the cells in such a way as to produce a secretion. Substances which, like secretin, are formed in one place and carried in the fluids of the body to another, there to elicit a response, are known as hormones or chemical messengers.

There have been other phases of work along the line of the chemical organization of organisms which would require too much space for a detailed consideration. One which seems to me significant is the determination of the various forms of starch grains and hemoglobin crystals by Reichert and Brown. These results tend to show that each species has its own peculiar physico-chemical constitution, that each organism has, in fact, a physico-chemical system of its own.

This work on the crystallography of the hemoglobins is an extension along another line of the work of Kossel on the chemical constitution of the protein molecule. According to Kossel, this consists of groups of aminoacids tied together, the nature of the acids and the number of each kind in the protein molecule depending upon the sort of protein selected for analysis. To a certain extent, the food value of any protein in animal nutrition depends upon the closeness of approximation of its qualitative and quantitative content of aminoacids to that of the tissues of the animal by which it is used for food.

Blackman, *loc. cit.*, in a most suggestive paper, has considered the chemical organization of the plant from the point of view of the application of the principles of chemical mechanics to the processes in living organisms. In the ten years that have elapsed since the publication of Blackman's address, the field of the application of the principles of chemical mechanics to the processes in living matter in general has been considerably extended.

It may be objected that we have never shown that the same principles cover the deportment of living and non-living matter, and the objection must be allowed. But neither do we know what principles cover the deportment of non-living matter in its entirety. Until we have gone farther in both fields, we have no rational basis for saying that living matter demands peculiar principles of its own. When it has been definitely shown that the principles of chemical mechanics, as they have been built up from the study of non-living matter, do not apply to living matter, or that no future principles which may be built up from the study of non-living matter will apply to living matter, we may be forced to take refuge in vitalism. But for the present, the free use of that old canon of logic known as William of Occam's razor[11]—the principle that the unnecessary supposition that things of a peculiar kind exist when the facts may be equally well explained on the supposition that no such things exist is unwarranted—should be freely applied in this connection.

But the chemical organization of the body, as has been stated, comprises but a part of the internal organization of the animal. We may now proceed to the consideration of the other phase, the nervous organization of the animal body.

THE NERVOUS MECHANISMS OF ANIMALS

The work on the nervous organization of the animal body has been done mostly on vertebrates, frogs, birds and mammals being chiefly used for experiment. Anatomical and pathological study has included the human nervous system as well. The influence of the Italian anatomist Rolando is reflected in the Rolandic area of the cerebral hemispheres, a term that still persists.

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[11] Entia non sunt multiplicandum praeter necessitatem. (Article "Razor," Century Dictionary.)

The French physiologists of the early part of the last century—Magendie, Le Gallois, Lorry, Desmoulins, Flourens and others—began or continued fundamental work on the organization of the central nervous system. Magendie published his text on physiology in 1816. So well were the chapters on the nervous system done that the Italian physiologist Luciani in 1893 wrote that he still found it a valuable and useful book. I can make the same statement in 1918. Magendie recognized the rôle of the central nervous system, not only in movement, but also in the maintenance of the attitudes of the body. He also stated that the division of the brain into its anatomical levels or segments such as cerebrum, cerebellum and the like was a purely artificial division so far as its physiology was concerned. In 1916 Luciani found occasion to emphasize the functional unity of the brain. Magendie also gave a statement of the mechanism of instincts which is so clear that I still regard it as one of our best.

Later in the century the discoveries in microscopy gave us definite ideas of the cellular structure of the nervous system. The accidental discovery of the Italian anatomist Golgi gave us a method of coating a nerve cell and all its processes with silver and enabled us to see clearly for the first time their exact form and extent. Anatomical and pathological studies have given us some knowledge of the relations of the fiber tracts in the brain and spinal cord. The anatomical phase of the subject is not yet complete and will not be for years to come.

The clinical observations of Broca on disorders of speech and of Hughlings Jackson on epileptic convulsions led to the early ideas of cerebral localization. The experimental results of Fritsch and Hitzig demonstrated that motor nerve fibers, excitation of which led to movements of particular groups of muscles, originated in definite regions of the cerebral cortex. Subsequent observations have shown that sensory fibers from particular sense organs have definitely localized end stations in particular regions of the cerebral cortex. There are still many unsettled questions in the field of cerebral localization, and much difference of opinion as to the degree or extent to which various functions rest upon an anatomically circumscribed basis in the cerebrum. Some would go so far as to deny cerebral localization in some of its essential aspects. Personally, however, I regard the theory of cerebral localization, and of localization in general within the nervous system, as well established in its main outlines.

The idea of functional integration, or physiological integration, as Herbert Spencer called it, has an application in the nervous organization as well as in the chemical organization of the organism. On its nervous side, it has been developed by Sherrington as the integrative action of the nervous system. Aristotle remarked that there was nothing in the mind, except what had come in through the senses. This conception can, I believe, be carried over to the side of the motor responses as well as to the mental. There are conditions, apparently, under which motor cells may discharge impulses without the previous access to them of afferent impulses over fibers of other nerve cells; but it does not appear to be the normal biological procedure. Afferent impulses seem necessary for the proper control of motor responses, or, to paraphrase a statement of Edinger, afferent impulses seem necessary to make the motor responses biologically adequate. The afferent impulses must be summed up or integrated somewhere within the central system before a biologically adequate motor response can occur.

The ordinary motor response to afferent impulses may be called a reflex response. The essential condition is that an afferent impulse shall find its way into the central nervous system and thence be "reflected back" over a motor pathway to a muscle or gland at the periphery. In recent years, Pawloff has shown that a sound of a given pitch or a particular color, or other external agent which, by itself, will not bring about a reflex secretion of the salivary glands of a dog, may be sufficient to excite such a reflex secretion if employed for a time every day in association with the same external agent which will normally bring about a reflex of saliva. If, for instance, a dog is shown a square of blue paper every day at the same time it is shown food, which will by itself elicit a reflex flow of saliva, after the lapse of a few weeks, the sight of the blue paper alone is sufficient to elicit the reflex flow of saliva. Pawloff has applied the term "conditioned reflexes" to such responses. From my own experimental results and from data now in the literature, I have concluded that practically all reflexes are conditioned reflexes, since, as I see the problem, a definite group of afferent impulses from different peripheral sources is necessary if any reflex response is to be biologically adequate. A certain definite set of conditions is necessary, therefore, for the elicitation of a definite reflex response. As may be gathered from these statements, I am inclined to extend the term reflexes to include a considerably larger group of phenomena than is covered by the older definition. On this point, I would sustain Loeb in his use of the term reflex in its wider meaning.

Beyond a certain point, the application of the general principles of chemical mechanics to the problems of the physiology of the central nervous system does not now seem possible. There has been, it is true, some change in the chemical composition of the nerves in the transition from frog to man. Certain protein substances which are coagulable at a temperature of 36° C. to 40° C.—a temperature below the ordinary body temperature of birds—appearing in the nerves of the frog are absent from the nerves of mammals and birds. But, in general terms, the same chemical foundation—the proteins as a group—is present, so far as we now know, in all nervous systems of vertebrates. Various other substances of a fatty nature, but all containing phosphorus or sulphur are also present in unmedullated nerve fibers, but we do not know either the exact nature of these substances in the nervous systems of various animal forms, nor how their variation affects the function of the nervous system at various levels in the evolutionary scale. The study of the metabolism of nerve cells and fibers is a chemical problem, and, to this extent, there is a chemical phase in the study of the nervous mechanism of the animal body. This chemical phase extends also to the study of disease in the nervous system, and through the work of Thudicum, Mott, Halliburton, Koch and others we have the beginnings of what we may hope will be an important phase of the study of the organization of the nervous system.

The study of the nature of the nerve impulse is also a chemical or a physical problem, or as now seems likely, a combination of the two.

The particular thing which characterizes the nervous system as a system is not its chemical organization, nor its rôle as a chemical mechanism, but its action as a coordinating or integrating mechanism. It is this integrative action which Sherrington has so luminously set forth in his writings. And it is by virtue of this integrative action in large part that man and the other animals express themselves by certain reactions arising in response to changes in the environment. Despite the objections that have been urged against it, and despite some obvious limitations, a modified anatomical basis seems the surest upon which to build at present. With, as I hope, a realization of its limitations, I may remark briefly upon the essential features of the method. Incidentally, the student of the scientific method may perhaps gain some idea of the diversity of the methods employed in physiology.[12]

We may regard the central nervous system as a physical rather than a chemical mechanism in the sense that, although some of the processes involved in the conduction of a nerve impulse and the excitation of a sensory ending, a central cell or an effector may be chemical, the relationships of afferent to efferent neurones are spatial rather than chemical, and our problem is not so much the problem of the nature of conduction and excitation as the problem of where the connection between incoming and outgoing impulses is made in the central system. The conduction paths traced out and the cell groups described by the anatomists afford a starting point, but they do not seem sufficient to answer all questions concerning functional relationships. The observation of the deportment of animals when some part of the central nervous system is lacking through disease or experiment and its comparison with the deportment of another animal of the same species when its nervous system and sense organs are intact is a necessary adjunct to purely anatomical study. The close and careful observation of the relation of the deportment of individuals of closely related species to slight differences in the organization of the nervous system has not been completed in most instances, but anatomical differences are observable in individuals of orders, genera or species less closely related. Observation of deportment of normal individuals, the modifications of deportment following disease or experimental procedures and anatomical description do not run unbroken parallel courses from the lowest animals to the highest; great gaps often exist in one or more lines of evidence, but some sort of a line may be traced from lowest to highest animals. Often, the three lines run parallel, and we see no apparent reason why, when all the gaps in all the lines of evidence are filled in by subsequent investigation, all should not run practically parallel throughout their courses.

The experimental method of the study of the function of the central nervous system is not particularly new. Some of the French experimenters of the early part of the last century have already been mentioned. But the method goes back even farther than this. Eckhard[13] refers to Pourfour du Petit's[14] research program of duplicating the various clinical manifestations resulting from disease of the brain in man by experimental procedures on animals. Needless to say, neither Petit nor those who have followed him even unto the present day have completed this program. But in the unhappy city and country in which Petit's program was first published, there has been inflicted upon the military and civilian population a series of experimental lesions of the nervous system by a race of supermen with bullet and shrapnel bomb, potato masher, grenade, bayonet, war club and high explosive, far transcending in variety and difficulty of execution the things which he contemplated doing on animals. And because of the employment of such methods, many of man's sufferings from the war and war conditions—deafness, blindness and shattered mentality—have been more noticeable in the present war than in other wars.[15]

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[12] See Sherrington, "Physiology; Its Scope and Method," in "Lectures on the Method of Science," edited by T. B. Strong, Oxford, 1904.

[13] Hermann's "Handbuch der Physiologie," Bd. 2, p. 106, Leipzig, 1874.

[14] "Lettres d'un Médicin à un Médecin de ses amis," Namur, 1710.

The experimentalist, although attacking the same general problem as the anatomist—the organization of the central nervous system—nevertheless has a somewhat different point of view. His object is not so much the mere acquisition of knowledge of the architecture of the nervous system—the knowledge of the location and form of certain cell groups, and the course of certain fiber tracts—as getting at the place where and the manner in which certain forces originating at the periphery are summed up or integrated in the central system to produce a definite, orderly and biologically adequate motor response. The method of the experimental neurologist or student of the function of the nervous system is the method of physics rather than the observational method of pure anatomy. The term integration may carry one back to his college days and the class room in integral calculus. And, so far as I understand their point of view, psychologists look at the problem in much the same way that the experimentalist does. The incompleteness of our knowledge is still as great as that of the anatomist. And the persistence in physiology of words of uncertain signification, by which we sometimes delude ourselves that we have an explanation of certain processes beyond the point where knowledge really ceases, still affords too much warrant for those who try to make the public believe that the words of uncertain meaning have a very clear and definite meaning. For only on some such basis can I understand the great vogue of the large and prosperous army of quacks who prey upon the unsuspecting or credulous public under the guise of faith healers, and the like.

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[15] Mott, F. W., "The Effect of High Explosives on the Nervous System," *Lancet*, February, 1916, and following issues.

Wilson, J. Gordon, "The Effects of Heavy Shell Fire on the Ear," Harvey Lectures, New York, 1917–1918. To be published in 1919.

Smith, G. Elliot, and Pear, T. H., "Shell Shock and Its Lessons," 2d ed., London and New York, 1917.

Babinski, J., et Froment, J., "Hystérie, Pithiatisme et Troubles Nerveux d'ordre Réflexe." 2me ed., Paris, 1918.

The reader should bear clearly in mind that we do not now know either the chemical or the nervous organization in its entirety. And in attributing any particular response or kind of deportment to either kind of organization, we are using the terms to signify what it does, as determined by observation and experiment rather than what it is.

Space does not permit a further presentation of the great mass of anatomical and functional detail which has been gathered in the course of years of study of the nervous system. The general reader who desires to get further information on a system whose study will, I believe, become of more and more interest and importance to the public in the years to come will find the salient points of the anatomy and physiology in the article "Brain" in the Encyclopædia Britannica. Professor F. W. Mott's excellent little book on "Nature and Nurture in Mental Development"[16] embodies the results of long years of careful study of problems of heredity of mental disease and other phases of the nervous system of interest to those who are interested in the social aspects of insanity and criminality.

Two minor aspects of internal organization remain to be considered; first, the organization of the heart, and then the organization of the cell. The first is of great importance for the well being of the higher animals, and the second for general biology.

THE MECHANISM OF COORDINATION OF THE HEART

The heart, to a certain degree, has an organization of its own. It is not a chemical organization in the sense that chemical substances must be carried or conveyed from one place to another in the body fluids, but a physical organization in that a wave of excitation is conducted over physical communications from one portion to another. It is now generally agreed that the impulses leading to the contraction of the various muscular groups of the heart originate in the Keith-Flack node (sino-auricular node) and are conducted to the muscles through the bundle of His (atrio-ventricular bundle) and the Purkinje substance. But whether the substance in the sino-auricular node in which the impulses originate is essentially nervous or muscular

[16] London and New York, 1914.

in character, or whether conduction in the atrio-ventricular bundle is over muscular or nervous tissue, or whether the Purkinje substance is nervous or more of the general nature of undifferentiated protoplasm are questions which, although subjects of controversy, do not particularly concern us here. The main point is that the organization of the heart is a physical organization approaching the general nervous organization more closely than the strictly chemical organization of the organism. The frequency of the heart beat may be changed by nervous and chemical influences.

THE ORGANIZATION OF THE CELL

The study of internal organization has extended also to the simplest organisms. Brücke (1861) called the cell the elementary organism and postulated an organization other than that represented by the visible structure. Whitman (1893) again insisted upon the importance of regarding the cell as an organism. Hofmeister some years later wrote on the chemical organization of the cell. But the study of the organization of the cell for many years has been predominantly a study of the chromatin material, principally from the point of view of the microscopist. This phase of the subject lies outside of the province of physiologist. The more recent work on the properties of cell membranes and the nature of colloids does, however, come within the realm of physiology, and is to be regarded as a part of our knowledge of the chemical organization of living matter in general. Its detailed discussion lies beyond the limits of this paper.

(*To be Continued*)